LICOS Centre for Institutions and Economic Performance

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LICOS Discussion Paper Series

Discussion Paper 343/2013

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Armed conflict and schooling in Rwanda: Digging deeper

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November 6, 2013

ABSTRACT

It has been shown that armed conflict in Rwanda had a strong negative impact on schooling. Huge gaps remain, however, in our understanding of its heterogeneous effects across subgroups and the underlying mechanisms. Relying on population census data, we show that – in contrast to previous findings - there is no leveling off, i.e. the negative impact is not stronger for non-poor and boys. We further demonstrate that slower grade progression as well as increased drop-outs explain the drop in primary schooling, while the drop in secondary schooling is driven by a decline in school initiation. Finally, our results reveal a spatial mismatch between commune-level genocide intensity and the drop in schooling. We test for several confounding factors - pre-war regional trends in schooling, migration, selective killings, and post-war assistance to genocide survivors - but find that none of these factors can fully account for the mismatch. We conjecture that the impact of armed conflict on schooling in Rwanda was nationwide, both because the disruption caused by the genocide was felt in every corner of the country and because - besides the genocide - other forms of violence took place in Rwanda in the nineties.

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1. Introduction

Several studies have looked at the impact of armed conflict on schooling. Most of these studies provide evidence for a negative impact (Alderman, Hoddinott and Kinsey, 2006; Akresh and de Walque, 2011; De Walque, 2004; Shemyakina, 2011; Chamarbagwala and Morán, 2011; Swee, 2009; Rodriguez and Sanchez, 2009). A much smaller set of studies reports a positive impact (Arcand and Wouabe, 2009; Valente, 2011). So far, the channels underlying these contrasting findings are poorly understood, and appear highly context-dependent. For instance, Valente's finding of a (small) positive effect on women's education of the Nepalese civil conflict is explained in terms of the relative low intensity of the conflict as well as the strong position of the Maoist insurgence against gender-discriminating traditions. Arcand and Wouabe (2009) refer instead to the post-war labor market, characterized by a low wage for non-skilled labor and high expected returns to investment in education, to explain prolonged schooling of cohorts affected by the Angolan civil war. Justino et al. (2013) - finding the effect of conflict in Timor Leste to be negative for boys but not for girls - conjecture that boys may be sent to work to compensate for a conflict-induced adverse income shock. Akresh and de Walque (2011), presenting results indicating that the impact of the Rwandan genocide was more negative for boys, argue that this could be due to a leveling-off effect, i.e. a negative shock bringing everybody to a low level of schooling, and therefore affecting disproportionately boys, who previously had enjoyed an advantage in terms of education. In contrast, both Shemyakina (2011) and Chamarbagwala and Móran (2010) find girls to be disproportionately affected by exposure to the Tajik conflict and Guatemalan civil war, respectively, which – as argued by these authors – may be due to the higher post-war returns to education for boys.

Most channels mentioned in these studies can be understood as operating through supply or demand factors for education. Supply-related factors include the destruction of schools, the targeting of teachers and a decrease in the government budget for education. Demand-related factors are very diverse, including population displacement, youth enrollment in the military or rebel groups, limited mobility because of fear and insecurity, changes in family structures (e.g. orphanhood), reduced life expectancy (leading to lower expected returns on human capital investment), increased poverty (leading, among others, to more child labor), and health and nutritional channels (reducing school performance and attendance). Justino (2010) and Buvinic et al. (2013) provide an overview. Which supply- and demand factors operate and their relative importance, is highly dependent on the specific setting considered. First, conflicts differ in intensity, duration, geographic spread, and in the nature of the destruction - taking a large human toll or mostly destroying infrastructures. Second, the socioeconomic context plays a role, for instance in terms of child labor, household coping mechanisms and gender-discrimination. Third, post-war policies vary widely, depending on aid flows and the capacity and nature of the post-war government in place.

A very different, but equally important, set of reasons underlying the mixed evidence relates to the specification and data used in the empirical analysis. As such, results may depend on the exact empirical specification adopted by the researchers, as well as on the type and quality of the conflict intensity measures used in the analysis, which can range from proxies such as dwelling destroyed to more detailed data on the number of battle events and human rights violations. Furthermore, the household survey data used in the analysis may or may not be representative at the national level or – more commonly – may not be representative across a

number of characteristics, such as gender and separate income strata, or at the level of small administrative subunits.

This paper contributes to the existing literature by studying the impact of armed conflict on schooling in Rwanda, a country located in Africa's turbulent Great Lakes Region and characterized by a long and complex conflict cycle in the nineties, including civil war in 1990-92 and 1994-98, the 1994 genocide, revenge killings, (counter-)insurgency operations in 1994-98 and a massive refugee crisis, which overall caused an death toll of almost 1.000.000 individuals.[1]

The first part of the paper builds on the widely cited study by Akresh and de Walque (2011) [2] - from now on referred to as AdW. In their study, AdW find that the Rwandan conflict caused an 18% drop in primary schooling, with the drop being stronger for boys and non-poor than for girls and poor. The authors attribute the drop in schooling to the genocide, by showing that the drop was more severe in regions where the genocide was more intense. We provide a replication of AdW, introducing three main changes to the analysis. First, we make use of a larger dataset. Whereas AdW rely on two waves of Demographic and Health household Surveys (DHS, 1992 and 2000), we make use of two waves of population census data (1991 and 2002). Both data sources bracket the main conflict events in Rwanda, but whereas the DHS is representative at the province level, the census data is representative for much smaller administrative units, for different income strata, age groups, and across gender. Secondly, we use

[1] It is estimated that the number of Tutsi killed during the genocide lies between 600,000 and 800,000, and that only 25% to 30% of the Tutsi population survived the 1994 genocide (Prunier, 1998; Verpoorten, 2005). The genocide targeted also moderate Hutu and an unknown number of Hutu became victims of revenge killings, other forms of violence, or fell victim to diseases in refugee camps (Des Forges, 1999). It is likely that the total death toll of all events related to the conflict cycle in the nineties amounts to 1,000,000 (Verpoorten, 2012). Moroever, it is estimated that approximately 2 million people were displaced, and the transitional justice system for genocide suspects led to the imprisonment of more than 100,000 civilians.

[2] According to Google Scholar, as of October 2013, the 2008 version of Akresh and de Walque's working paper was cited 110 times. The paper exists as an IZA discussion paper (IZA DP No. 3516, 2008), a World Bank Policy Research Paper (WPS4606, 2008), and a Households in Conflict Network Paper (HiCN Working Paper 47, 2008). The version we refer to is the latest update that is available on the personal website of Richard Akresh, dated February 2011.

more detailed conflict measures defined at the level of 145 administrative communes instead of at the level of the 11 provinces. Finally, in contrast to AdW, we include all constitutive components of the interaction terms when studying the heterogeneous impact across gender and income. This is the correct way to proceed as argued among others by Brambor et al. (2006). Our results confirm the finding of a large drop in schooling, but not the results regarding the impact on poor versus non-poor, and girls versus boys. Also contrary to AdW, our results indicate that the drop in schooling cannot readily be attributed to the localized impact of the genocide.

Besides this broad replication of AdW, we provide novel evidence on the impact of conflict on secondary schooling and on the relative importance of school initiation, grade progression and drop-outs. Finally, we investigate the reasons underlying the spatial mismatch between commune-level genocide intensity and the drop in schooling. After demonstrating that a number of potentially confounding factors cannot fully account for the mismatch, we discuss the possibility that the impact of armed conflict on schooling in Rwanda was nationwide, both because the disruption caused by the genocide was felt in every corner of the country and because - besides the genocide - other forms of violence took place in Rwanda in the nineties.

In sum, the contributions of this paper are fourfold. First, we show that by resorting to an improved empirical strategy and a more rich and representative dataset, our broad replication of AdW yield different and (we argue) more correct results. Second, the new results we provide are a contribution in itself, as they add to the growing body of literature that investigates the heterogeneous effect of armed conflict across gender and income strata. Third, thanks to the rich data at our disposal, we are able to study for the first time the different impact of conflict on primary versus secondary school, as well as the relative importance of school initiation, grade progression and drop-outs. Finally, we add to the existing literature on the Rwandan genocide,

by carefully investigating the relative importance of the localized effects of the genocide (versus nationwide effects) in explaining the decline in schooling.

In the next section, we demonstrate the reliability of the Rwandan population census data, by showing that the census provides an exact match with population data from another source, and that the basic result of AdW - an 18% drop in primary schooling - can be replicated almost perfectly. In section 3 we study the sensitivity of this result across the choice of age groups and quantify the drop in secondary schooling. Section 4 looks at the variation of the schooling deficit across income and gender, providing evidence against the 'leveling-off hypothesis' put forward by AdW. Section 5 examines the relative role of school initiation, slow grade progression and dropouts. In section 6 we demonstrate that the spatial distribution of the drop in schooling cannot be attributed to the localized effects of the 1994 genocide. Section 7 and 8 investigate why this is the case. Section 9 concludes.

2. The reliability of the Rwandan population census data and the replication of the basic result of AdW

The DHS are firmly established in empirical research as a reliable source of information, mainly because of its standard and transparent approaches for data collection, cleaning and coding. In contrast, population census data do not enjoy a good reputation because the collection method and practice vary a lot across countries and census definitions of citizenship and ethnicity are often highly politicized. In the case of Rwanda, ethnicity is indeed politicized, which is reflected in the omission of ethnic identity in the 2002 population census, in line with the public rhetoric of national unity after the 1994 genocide. But, leaving ethnicity aside, the Rwandan census data turns out to be very reliable. Verpoorten (2005) compares the 1991 Rwandan census data with

1990 population data from the local administration of Gikongoro province and finds an almost exact match of the total number of women and men, which is indicative for the quality of both sources as they were collected independently of each other.[3]

Another finding in support of the reliability of the Rwandan census data, is that - when repeating the basic empirical estimation of AdW with population census data instead of DHS data - we find a very similar estimate for the impact of conflict on schooling.

AdW study years of education of a young (6-15) and an older (16-35) age cohort in the 1992 and 2000 DHS survey, and conclude that children exposed to armed conflict completed close to one-half year less education, corresponding to a 18.3% drop relative to the average educational achievement. For the replication of AdW, we use the exact same empirical set-up, which consists of a difference-in-difference (DD) estimation of years of education, in which the identification of the conflict impact stems from exposure of the young age cohort in the 2000 DHS to armed conflict at schooling age. The empirical equation for the DD estimate is:

[3] The 1991 national population census was collected in August 1991 by the national statistical office in collaboration with the UN, while the local population data used was last updated in December 1990 by the local administrative staff. The local administrative staff recorded all births, deaths, immigrations and emigrations; and had to file reports on its population four times a year (Verpoorten, 2005). While the number of men and women is almost identical across both sources, the share of Tutsi in the population in the 1991 national census data is almost 40% lower than the share in the 1990 local population data. Since the local authorities had no reason to over-report the proportion of Tutsi in their population, and civilians themselves could not misreport to the local authorities as their ethnicity was known by their fellow community members, this indicates that the proportion of Tutsi in the national census was under-reported.

$$Y_{it} = \alpha_0 + \alpha_1(T_t \times young_i) + \alpha_2 T_t + \alpha_3 young_i + \varepsilon_{it} \qquad \textit{(Eq. 1)}$$

WITH

Y_{it} : years of schooling of individual i at time t

T_t : indicator variable for being in the postwar round

$young_i$: indicator variable for being in the young age cohort

ε_{it} : idiosyncratic error

The summary statistics for the DHS variables are reported in Panel A of Table 1.

---- Table 1 about here ----

Restricting observations to individuals aged 6 to 35, leaves 18,314 and 27,086 observations for the 1992 and 2000 DHS, respectively. AdW find a DD estimator (α_1) equal to -0.555; significant at the 1 percent level. In a narrow replication using DHS data, we find a very similar result, with a DD of -0.504. In a broad replication, relying on a 10% random draw of the 1991 and 2002 censuses[4] with observations on 441,870 and 465,156 individuals aged 6-35, we find a DD estimate of -0.584, corresponding to a 16.4% drop in years of schooling[5] (the summary statistics for the census variables are reported in Panel B of Table 1). This estimate is very close to the AdW result, despite slightly different sample years: 1992 and 2000 for the DHS versus 1991 and 2002 for the census data. Table 2 gives an overview of these results.

---- Table 2 about here ----

[4] The 10% draw of the 1991 census is made available by Minnesota Population Center (2010). The same source also provides access to a 10% draw of the 2002 census, but this sample does not include the names of the administrative sectors. We therefore take a new 10% random draw directly from the full 2002 census. Results remain qualitatively the same when considering the full census.

[5] The proportional change in schooling is derived as the DD estimate when the dependent variable equals the natural logarithm of years of schooling.

In a next step, we follow AdW by expanding Eq.1 to include household level controls *X* as well as province and age fixed effects:

$$Y_{itp} = \alpha_0 + \alpha_1(T_t \times young_i) + \alpha_2 T_t + \alpha_3 young_i + X_i\Delta + \varpi_i + \pi_p + \varepsilon_{itp} \quad \textit{(Eq. 2)}$$

WITH THE ADDITIONAL VARIABLES DEFINED AS

X_i : household and individual level control variables

ϖ_i : age fixed effects

π_p : province fixed effects

We take care to include the same household level controls as AdW: an indicator variable for female, for non-poor households[6], the age of the household head, the highest education level of any household member, the number of children under 5, and an indicator variable for rural areas. The DD estimate of the augmented empirical equation reported by AdW is -0.421***. In our narrow replication using DHS data we find again a similar estimated coefficient of -0.494***. The broad replication using census data yields an estimate of -0.660***. In proportional terms, this latter figure amounts to a drop of 18.6%, which matches almost perfectly the 18.3% estimate of AdW.

In sum, the analysis presented in this section confirms the main result of AdW of a large drop in schooling and demonstrates that the DD estimates are very similar across the DHS and census data. In addition, the coefficients on the control variables *X* are also similar. This is an indication for the reliability of the census data and lends support to their use in our exploration of the channels that account for the schooling deficit.

[6] The indicator variable *non-poor* indicates if the individual has more assets than the population mean. The assets include piped running water, refrigerator, radio, finished floor, bicycle, motorcycle, and car (AdW, 2011).

3. Alternative age cohorts and the drop in secondary schooling

Before we start investigating the channels underlying the drop in schooling, we repeat the basic analysis with different choices of age groups. We do so for two reasons: first, to demonstrate the sensitivity of our results to alternative cut-off ages; and second, to inform about the impacts on secondary schooling.

Above, we have taken the age group 16-35 as a control group for studying the impact of conflict on the young cohort, 6-15. Doing so, we follow the baseline specification of AdW [7], but strictly speaking, the age groups used in our broad replication are not a one-to-one match with those of AdW. One could argue that a closer match to the age cohort used in AdW is the age cohort 8-37 in the 2002 census, since individuals aged 8-37 in 2002 were aged 6-35 in 2000, the DHS survey year. Therefore, we repeat our broad replication of AdW using the age cohort 8-17 as the young cohort and 18-37 as the old cohort. The results, shown in Table 3, are qualitatively the same, but quantitatively larger, with a DD estimate of -0.931*** for Eq. 1, corresponding to a 21.3% drop in years of schooling.

---- Table 3 about here ----

The old age groups (16-35, 18-37) used so far as control groups include individuals that were at schooling age when the genocide broke out. For instance, individuals aged 16 to 22 in 2002 were aged 8 to 14 in 1994. At that age, many were still attending primary school[8], and their schooling attainment may thus have been affected by the conflict. This is confirmed by Figure 1 which shows that years of schooling in 2002 is lower than in 1991 for individuals up to 22 years

[7] In a number of robustness checks AdW find similar results when using alternative cutoff ages for the young and old cohorts. In their robustness checks, they define the young cohort as aged 6 to 12, 6 to 18, 6 to 21, and 6 to 24; and the old cohort as aged 16 to 25, 16 to 30, 16 to 40, 16 to 45, 16 to 50, and 16 to 55.

[8] According to the 1991 census, 39% of the children aged 8 to 14 are enrolled in school, with 78% of them being enrolled in primary school.

old. In Column (2) of Table 3 we therefore repeat our estimation using the expanded young age cohort (6-22). We find a drop in years of schooling of -1.189***, corresponding to a proportional decrease of 25.0%.

---- Figure 1 about here ----

This result suggests that, besides affecting primary schooling, the conflict may have affected secondary schooling. To study the impact on secondary schooling, we compare the age group 14-22, which in 1991 represented 73% of the student population enrolled in secondary schooling[9], with the age group 23-35. We find a DD estimate of -1.083***, representing a decrease of 17.6% of secondary schooling years.

In sum, this section indicates that the overall drop in schooling estimated using the census data varies between 18.6% and 25.0% depending on the choice of the age groups. The age cohort 16-35 may not be the best choice as a control group because it includes individuals who were at primary schooling age at the time of the armed conflict. Even if the control group would be restricted to individuals aged above primary schooling age, the impact of schooling may still be underestimated, as secondary schooling was also affected by armed conflict. Despite the caveats of the age groups 6-15 and 16-35 we use these groups in our baseline results below so to allow direct comparison with AdW. Given this choice, our estimated effect of conflict on schooling attainments should be considered as a lower bound. We also re-run all our regressions using the cohorts 8-17 and 6-22 as young age cohorts. All our results remain in a qualitative sense.

[9] Like most African countries, Rwanda is characterized by large variations in the age of the students attending the same class, which complicates the demarcation of secondary schooling age. In 1991, 96% of students enrolled in secondary school were between 12 and 22 years, but the majority of those aged 12 and 13 were still in primary. This is why we set the cutoff age at 14.

4. Leveling-off versus no leveling-off

(a) Non-poor versus poor

AdW set out to evaluate whether conflict affects years of education differently across the poor and non-poor in the DHS samples. For this purpose, they add a three-way interaction term to Eq. 2, in which the treated group (young age cohort in the 2000 DHS) is interacted with a household-level indicator variable for being non-poor (*non-poor*young*T*). However, their approach has two main limitations.

First of all, AdW do not include the constitutive components of the interaction term, i.e. they do not include the terms *non-poor*young* and *non-poor*T.* As pointed out by Brambor et al. (2006), the omission of constitutive components of interaction terms may severely bias the estimates. The authors find a large negative DDD estimate of -1.223*** on their three-way interaction term, and conclude that *"Somewhat surprisingly, it is among the children in non-poor households in which the negative shock is strongest."* (p. 12). We can replicate this result almost exactly using the DHS data, provided that we omit the constitutive terms *non-poor*young* and *non-poor*T* (not reported). When including these terms, however, we obtain an estimated coefficient of -0.399***, indicating a much smaller additional negative impact on schooling for the non-poor. When we perform the broad replication using the census data, the estimated DDD coefficient is no longer significantly different from zero, at 0.013. These results are reported in Table 4.

---- Table 4 about here ----

Even when including the constitutive terms, however, estimates may be biased due to several confounding factors. For instance, poverty may itself be a determinant of conflict intensity, or the non-poor may be characterized by different trends in pre-war schooling. To

solve for the confounding factors, one would need to include a rich set of control variables and/or properly instrument for conflict intensity, requiring rich data and/or a valid instrument - both of which are in short supply. We will therefore not be able to provide a clear answer to whether conflicts disproportionately affected poor households in terms of years of education.

What we can do is highlight the importance of confounding factors and the need to qualify the results. Take for instance the different trends in schooling across urban and rural households, shown in Figure 2.

---- Figure 2 about here ----

Since the urban areas are home to many of the non-poor, these different trends are associated with different trends across poor and non-poor; using the measure of poverty as presented by AdW, 42% of the people living in rural areas were non-poor in 1991, compared to 81% of the people living in urban areas. When we run our regression including an indicator variable for urban communes [10] and the full set of interaction terms *urban*T*, *urban*young* and *urban*young*T*, the coefficient on the three-way interaction term *poor*young*T*, albeit of small magnitude, turns positive and significant at the 10% level, suggesting that, if anything, conflict has a stronger negative effect on poor households.

In sum, the result of AdW of a higher schooling deficit for non-poor fades away when adding the constitutive components of the interaction terms and is reverted when accounting for part of the non-randomness of poverty by including a set of urban time trends and interaction terms.

[10] The urban indicator variable takes the value one for the communes of Kigali City, the provincial capitals and a number of other secondary urban centers.

(b) Boys versus girls

Conflict cannot turn boys into girls, or girls into boys; and sex is not clustered in space. This makes it relatively easy to study the heterogeneous impact of conflict across boys and girls, as the three-way interaction term *female*young*T* can be interpreted unambiguously.

AdW add this term to Eq. 2 and report a positive DDD estimate of 0.219***, suggesting that the impact of the conflict on the schooling of girls is less negative than the impact for boys. This points to a leveling off effect: *"A potential reason might be that female schooling outcomes were lower initially, and so they had less to lose after the genocide"* (AdW, p.12).

---- Table 5 ----

However, also in this case, two out of the three constitutive terms of the interaction effect are erroneously omitted (*female*young* and *female*T*). In a narrow replication on the basis of DHS data and omitting the constitutive terms of the DDD we also find a positive coefficient (0.148***, not reported). In a correct specification we find instead an estimated coefficient of -0.039 and, when using the census data, we obtain a negative coefficient that is significantly different from zero, -0.162***, pointing to an additional schooling deficit for girls. These results are reported in Table 5. When running the regressions on subsamples, for boys and girls separately, we find a DD estimate of -0.596*** for boys and -0.727*** for girls; indicating that the conflict led to a drop in schooling of 15.7% for boys, compared to a drop of 21.3% for girls (not reported in the Table). The result is robust to changes in the age thresholds considered: the coefficient of the DDD term remains negative and significant at 1% when we compare the cohort 8-17 to 18-37 (-0.130***, not reported) and 6-15 to 23-35 (-0.126***, not reported). When comparing the cohorts 6-22 to 23-35 the estimated coefficient weakens to -0.079**, and significance disappears when we focus only on secondary schooling, comparing cohort 14-22 to

23-35 (coefficient -0.43). These results suggest that the negative effect was stronger for girls at primary schooling age, but not for girls at secondary schooling age, which is also confirmed by Figure A1, in which we report the estimated DDD coefficient for each age, from 6 to 20.

As the DDD estimate is unlikely to be affected by confounding factors, it should remain stable when adding the urban factors (*urban*T*, *urban*young* and *urban*young*T*). We indeed find that when adding these terms, the estimated DDD coefficient only changes marginally from -0.162*** to -0.172***.

In sum, the result of AdW of a higher schooling deficit for boys is reverted when adding the constitutive components of the interaction terms. Our findings point to an additional negative conflict impact on the schooling of girls, and especially so for girls at primary schooling age.

5. School initiation, drop-out or slow grade progression?

The drop in schooling can be due to (H1) children who do not initiate school, (H2) children who drop out of school, or (H3) children who slowly progress through grades. AdW investigate the role of school initiation (H1) by studying grade completion separately for grades 1 to 6. They find that, first-grade completion is very similar across 1992 and 2000 and conclude that "*the schooling deficit of the cohorts who were of schooling age after 1994 is not so much due to a lack of access to education, since the differences for the first grade are minimal, but rather to difficulties to progress through the grades or continue attending school.*" (p. 7-8).

AdW do not investigate the relative importance of (H2) dropouts and (H3) repetition.[11] We can shed more light on this because the 1991 and 2002 censuses contain information on the

[11] On page 14, AdW write that: "*... with the available data, it is not possible to determine definitively if this impact of genocide on grade progression is due to children repeating grades or dropping out of school after the first few grades."*

past and present schooling status of the individuals in the sample. More specifically, for each person we know whether he or she has ever attended school and whether he or she still is a student.[12] Using these two pieces of information, we construct the variables *everbeentoschool* and *student.*

We first produce a set of simple figures. Figure 3a displays the shares of individuals who ever attended school by age and year. Comparing these shares across 1991 and 2002, it is clear that, in 2002,individuals across all ages of the age group 6-35 were more likely to have ever attended school, which supports the finding of AdW that school initiation (H1) is not a major issue.

Figure 3b displays the share of individuals aged 12 or above that completed primary school, among the population that is no longer in school but that has attended school in the past. The figure shows that, in the census year after the conflict, more students dropped out before completing primary education, providing support for (H2).

Finally, if slow grade progression (H3) is among the causes of the observed drop in schooling, we should find that students in 2002 are older than students in 1991, conditional on the grade they attend. This is shown in Figure 3c, which gives the average age of students across twelve different grades. The average age of students is higher in 2002 than in 1991 for each of the six primary grades (1-6) and across the three years of lower secondary schooling (7-9), but not for upper secondary schooling (grades 10-12). For instance, students who attend the final grade of primary school are on average 15.9 years old in 2002 compared to 13.4 in 1991.

[12] More precisely, census data only records the years of schooling completed by each individual. We therefore approximate the fact of ever attending school with the completion of the first year of primary school. Also, the student/non student status is available only for individuals aged 10 or above in the 1991 census.

Combined, these figures suggest that drop-outs (H2) as well as grade repetition (H3) are driving the result of a decrease in primary schooling, while school initiation (H1) is not a major issue.

---- Figure 3 about here ----

We study this more formally. First, focusing on school initiation (H1), we repeat the estimation of Eq. 2 when replacing years of schooling by *everbeentoschool* as a dependent variable.[13] Column (1) of Table 6a shows that the estimated coefficient on the interaction term *young*T* is not significantly different from zero, at -0.007, indicating that the young cohort in 2002 was not less likely to have completed at least one year of school (compared to the older cohort in 2002 and relative to the difference between the young and old cohort in 1991).

To study whether dropouts (H2) played a role, we take primary school completion as our dependent variable. Focusing on those individuals that ever attended school but are no longer enrolled in primary school, we redefine the young cohort as those aged 14 to 22 and the old cohort as those aged 23-35.[14] Estimates reported in Column (2) of Table 6a show that in 2002, students in this young age cohort were 28.5% less likely to have completed primary school compared to the young cohort in 1991.

---- Table 6 about here ----

To study the case of slow progress (H3), we restrict our analysis to individuals enrolled in grades 1-6, and to the age group 10-18 because information on student status is available only

[13] Although this variable is binary, we keep the linear estimation model, because it is more traceable and any possible bias will be small given the large number of observations. Results are in any case all confirmed when using a Probit model.

[14] According to the 1991 census, 93% of the students enrolled in primary school were younger than 14. Results are in any case very robust to small changes in the definition of the cohorts.

from age 10 onwards, and because above age 18 there are very few primary school students[15]. We introduce an individual's age as the dependent variable in the following equation:

$$A_{itg} = \beta_0 + \beta_1 T_t + X_i \Delta' + \Gamma_g + \pi_p' + \varepsilon_{itg}' \qquad \textit{(Eq. 3)}$$

with T_t, X_i, and π_p defined as in Eq. (1) & (2)

A_{it} : age of individual i at time t in grade g

Γ_g : grade fixed effects

η_{itg} : idiosyncratic error

The coefficient of interest is β_1. It indicates whether, for a given grade, students are on average older in the post-war population than in the pre-war population. The estimate for β_1, 1.708***, provides clear evidence for slow grade progression. On average students in 2002 were over one and a half years older than students in the same grade in 1991.

Panel B of Table 6 shows the results for secondary schooling. In sharp contrast to primary schooling, but in line with the patterns in Figures 3abc, the results indicate that school initiation is the most salient factor explaining the drop in secondary schooling.[16] In Column (1) only individuals that completed primary school are considered. The coefficient indicates that in 2002, individuals in the age cohort 14-22[17] were 40% less likely to complete the first year of secondary school compared to the same cohort in 1991 and relative to the difference between that cohort and the old one in the census rounds. On a positive note, results in the second column indicate that those who started secondary schooling were not less likely to finish it (if anything,

[15] According to the census rounds, the share of individuals over 18 attending primary school is less than 0.5% in 1991 and less than 2% in 2002. Once again, results are very robust to small changes in the age thresholds.
[16] As the census only records the years of schooling completed by each individual, also in this case we approximate enrolment in secondary school with the completion of the first year of secondary school.
[17] See Section 3 for a justification of the choice of the 14-22 age category.

they were more likely to do so), and this despite evidence for a slightly slower grade progression (last column)[18].

In order to test whether these effects are the same for boys and girls, we add to our regression the interactions with the *female* dummy. Results reported in Table A1 in Appendix reveal a heterogeneous impact of conflict on school initiation, dropouts and grade progression across gender However, the differences are generally small and our previous conclusions on the relative importance of the three factors hold for both boys and girls. The most significant difference concerns the increase in dropouts from primary school, which is estimated to be about 50% higher for girls than for boys. The difference in primary school enrolment is also significant and reveals a somewhat negative effect for girls, but not for boys; even if the effects are quantitatively small.

6. Attribution to genocide: contrasting findings

Among the events in Rwanda's conflict cycle of the nineties - civil war, genocide, revenge killings, (counter-)insurgency operations and a massive refugee crisis - the genocide stood out with a death toll of close to 800,000 in barely 100 days, between April and July 1994. AdW test whether the drop in schooling can be attributed to the genocide by estimating a difference-in-difference-in-difference (DDD) model in which the treatment group (young age cohort in the post-war round) is interacted with a measure of genocide intensity. In other words, they augment Eq.2 with the term *T*young*genocide_intensity* as well as all its constitutive components.

[18] The choice of the lower limit of 18 is based on the fact that according to both the 1991 and 2002 census virtually nobody completed secondary schooling before that age. The choice of the upper limit of 26 is instead more arbitrary. In any case, less than 0.5% of individuals enrolled in secondary school in 1991 were above 26. Results are very robust to small changes in the definition of the upper limit.

AdW consider three different genocide intensity measures: the proportion of days during which killings occurred in a province in the months April-June 1994 (Measure A), an indicator variable taking one for the three provinces with the highest number of killings in 1994 (Measure B), and the number of mass graves and memorials per province (Measure C)[19]. The DDD estimates obtained by AdW – reported in Panel A of Table 7 - are all negative and significant at the 10% level (A:-0.024*, B:-0.329*, C:-0.023*), pointing to a significantly stronger negative impact on schooling in provinces where the genocide intensity was higher.

We cannot replicate these results. As Panel B of Table 7 shows, in our narrow replication, using the same data, variables and specification, we obtain very different coefficients: A:0.744**, B:-0.892*** and C:0.005. It is not clear what accounts for these differences[20]. Estimates are also very different when we estimate the DDD model using the population census data; the coefficients - reported in Panel C of Table 7 - are all close to zero (A:0.003**, B:0.094, C:0.001).

---- Table 7 about here ----

In an attempt to address the potential endogeneity of genocide intensity, AdW perform an IV estimation. Using the province-level distance to the Ugandan border as an instrument, AdW report IV coefficients that are in line with their OLS results. Once again, we obtain very different results – mostly not statistically significant - both in our narrow and for our broad replication[21].

[19] Measure A and B are taken from the genodynamics project (Davenport and Stam, 2009); measure C is taken from the Yale Genocide Studies website (http://www.yale.edu/gsp/rwanda/)

[20] Dropping the province fixed effects, or the constitutive components - one by one and in pairs – does not bring the replication results closer to the results presented by AdW.

[21] Results are available on request. We do not discuss them in detail because the exclusion restriction is in any case unlikely to hold. To defend the choice for the instrument, the authors argue that "*Provinces close to the Ugandan border were reached faster by the RPF who stopped the killings perpetrated by the Interhamwe militia, and therefore these provinces are more likely to be low war intensity provinces*" (p.11). Although this instrument is relevant, i.e. it is sufficiently correlated with 1994 killings, it is unlikely to be exogenous, as the Southern part of the

Leaving aside the comparison with the results of AdW, our failure to establish a direct link between the drop in schooling and the genocide may be due to the fact that the conflict intensity measures considered so far are all defined at the province level and may be too crude to properly capture variation in genocide intensity. We address this issue by turning to four finer measures, defined at the level of the 145 administrative communes: the proportion of Tutsi in the pre-war population, the share of genocide suspects, the number of mass graves, and the distance to the nearest mass grave calculated from the commune centroid.[22] Table 8 reports the estimates: none of the measures yields a DDD that is significantly different from zero and of the expected negative sign.[23]

In sum, the DDD model does not attribute the observed drop in schooling to the genocide, even when using finer genocide intensity measure and a larger and more representative dataset.

---- Table 8 about here ----

7. Why can't we attribute the drop in schooling to genocide?

We investigate the possibility that the attribution of the drop in schooling to the genocide (the DDD estimate) is confounded by pre-war regional trends, migration, selective killings, or post-war assistance to genocide survivors. For ease of exposition, we only discuss results in which we

country was also characterized by a higher concentration of Tutsi, which traditionally enjoyed higher levels of education (see also our discussion in Section 7).

[22] The first of these measures is calculated from the 1991 Rwandan population census, the second from the records of the Gacaca (the transitional justice system for genocide suspects), and the latter two from a map taken from the Yale Genocide Studies website.

[23] To investigate whether endogeneity might be biasing these estimates, we repeat the regression instrumenting the conflict variables (and their interactions) with the commune-level (log) distance to the border with Uganda, following the approach suggested by Wooldridge (2002, p.236). The qualitative results remain the same (not reported). Finally, we repeat the regression including commune fixed effects to limit the likelihood of bias stemming from omitted time-invariant factors, and find again similar results (not reported).

consider the share of Tutsi living in the commune in 1991 as the proxy for genocide intensity, but results are similar whenever one of the alternative proxies is considered (not reported, but available on request).

(a) Pre-war regional time trends in schooling

The DDD takes the pre-war population as a control group, implicitly assuming that conflict intensity does not relate to pre-war trends in schooling. This assumption is violated if both conflict and schooling trends are affected by an unobserved common factor. One such factor may be the identity of the political leader[24]. In Rwanda, prior to independence, the Tutsi monarchy was dominating and the areas around the capital of the Tutsi monarchy ('Nyanza', located on the intersection of Gitarama and Butare) were flourishing. The ethnicity of the leader changed from Tutsi to Hutu in 1959, and discrimination against Tutsi increased, especially during the Habyarimana regime (1973-1994) when favors were directed to the core of Habyarimana's supporters, residing in the provinces of Gisenyi and Ruhengeri, in the Northwest of the country (Des Forges, 1999).[25]

Clientelism and switching powers may therefore account for pre-war differences in schooling trends across the country.[26] To check whether these pre-war trends are confounding our DDD results, we add a complete set of province trends and province interaction terms to the

[24] Kudamatsu (2009) and Franck and Rainer (2012), among others, show that there is a strong link between the identity of political leaders and the development of the regions that are favored by them.

[25] About Habyarimana's political party, the Mouvement Révolutionnaire Nationale pour le Dévelopment (MRND) Des Forges (1999, p.45) writes: *"…the MRND had regulated access to government-supported high schools, supposedly assigning places according to quotas for ethnic and regional groups. The quotas were both inaccurately computed and unfairly applied, favoring children from the Northwest or those whose families could pay in money or other benefits for access to education."*

[26] Figure A2 in Appendix shows that whereas individuals in the old age cohort in Butare and Gitarama had higher schooling in 1991 than their counterparts in Gisenyi and Ruhengeri, the difference is minimal for the young cohort in 1991. This pattern is consistent with a catch-up of schooling in the stronghold of Habyarimana regime. The pattern remains even when removing Tutsi from the sample, suggesting that - besides ethnicity - regionalism played a role.

DDD regression model (*i.province*young*postwar*, *i.province*young*, *i.province*postwar*). Panel A of Table 9 reports the results: the DDD estimate remains statistically indistinguishable from zero.

(b) Nonrandom displacement

The aftermath of the genocide was characterized by a huge refugee crisis. An estimated two million Rwandans were displaced in 1994, with the majority of them resettling only in the period 1996-98 (Des Forges, 1999). If especially highly educated adults moved out of the most affected communes, the gap between the young and old cohort remaining in those communes would appear smaller than what would have been otherwise, thus biasing our DDD estimate towards zero.[27]

The census data include information on place of birth, previous residence, current residence and time at current residence, allowing us to trace an individual's migration history. To gauge whether migration is causing a bias we assign all individuals who moved between 1994 and 2002 to their previous commune of residence and we re-estimate the DDD model.[28] The results, reported in Panel B of Table 9, show that the DDD coefficient remains statistically insignificant.[29]

[27] Figure A3 in Appendix shows that adults who migrated between 1994 and 2002 had on average higher levels of education compared to non-migrants.

[28] In 2001 Rwanda underwent a large administrative reform that transformed its 145 communes into 106 districts. The 2002 census only provides previous district of residence. For those districts that overlap with different communes we arbitrarily assign the individual to one of the communes. Results are robust to dropping these observations and only re-assigning individuals that migrated from one of the 20 districts perfectly matching previous communes.

[29] We cannot account for individuals who left Rwanda after the genocide and did not return, as they are not included in the 2002 census. Again, if especially well educated adults left heavily genocide-affected areas to go abroad and did not return, the gap between post-war young and old cohorts would be reduced, biasing our DDD estimate toward zero.

---- Table 9 about here ----

(c) Selective killings

de Walque & Verwimp (2009) demonstrate that the probability of being killed in the genocide was relatively higher for men, for the well-educated and for Tutsi, and was highest among the well-educated Tutsi male population. Figure 4 shows that Tutsi had on average more years of schooling than Hutu in 1991. Both the targeted killing of Tutsi adults and of other well-educated adults would thus bias downward our DD and DDD estimates, since it would reduce the gap between the schooling of the young and old age cohorts in the post-war population, and especially so in the provinces and communes with high genocide intensity.

---- Figure 4 about here ----

As it has been estimated that approximately 75% of Tutsi were killed during the genocide, we gauge the magnitude of the bias by re-running our estimations after randomly removing 75% of Tutsi from the 1991 population, i.e. after artificially introducing in the 1991 census a selection similar to the one caused by the genocide in 1994. This artificial manipulation of the sample, which results in the loss of around 3% of the observation, pushes the DDD coefficient down to -0.213, but leaves it statistically insignificant (Panel C of Table 9). Moreover, the large and significant coefficient of the two-way interaction term (*young*post-war*) remains, indicating that the localized effects of the genocide cannot fully account for the drop in schooling, even when controlling for the selective aspect of the killings.[30]

[30] A stronger test would be to drop the 75% best educated Tutsi from the old age cohort, instead of a random sample. Doing so results in a negative and significant DDD coefficient of -0.836*** (not reported), indicating that the selective aspect of the killings has indeed the potential to mask the link between genocide intensity and the drop in schooling. However, it still cannot account for the large drop in schooling, because the DDD remains

(d) Post-war assistance to genocide survivors

In the aftermath of conflict, many targeted assistance programs for genocide survivors were launched. For instance, the FARG (Fonds d'Assistance aux Rescapés du Génocide) supports genocide survivors with allowances, health insurance, housing and school fees, among others. Table 10 – taken from the FARG website[31] - provides an overview of school fees paid throughout the years 1998 to 2010. In 1998, barely four years after the genocide, FARG was already awarding scholarships for secondary schooling to 24,000 students, which is a sizeable share of genocide survivors at schooling age.[32] We lack detailed data on the other forms of support, but these figures are suggestive of the amount of resources mobilized to support genocide survivors.[33]

---- Table 10 about here ----

The support received by genocide survivors may confound the link between the intensity of the genocide and the drop in schooling. Figure 5 provides suggestive evidence. Relying on data from a nationally representative survey collected in 1999/2000 (EICV1), it shows a markedly positive relationship between the share of students that report enjoying a scholarship in 2000 and the province-level share of Tutsi in the pre-war population.[34] The strong link between genocide and the scholarship program is also underscored by the fact that most of its beneficiaries (63%) are paternal orphans. The channeling of scholarships to genocide orphans

quantitatively small (the average share of Tutsi in the sample is equal to 0.11) and, above all, because the large and significant coefficient of the two-way interaction term (*young*post-war*) remains virtually unaffected.

[31] http://www.farg.gov.rw/

[32] It has been estimated that in total 300,000 Tutsi survived (Prunier, 1998) – among which about 20% would have been at secondary schooling age in 1998.

[33] Although the scholarship program of the FARG targets secondary schooling, the foresight of access to secondary schooling may positively affect the decision to send children to primary school. Besides, primary schooling may be funded by the cash allowances, which amount to RWF 5000 monthly, and were received by about 30,000 people in 2011.

[34] The relationship holds for students in the age group 6-35, 6-15 and 16-35.

could account for a surprising finding in the census data, i.e. the small difference in schooling between orphans and non-orphans in provinces with a high share of Tutsi, while this difference is larger in provinces with low shares of Tutsi (see Figure A4 in Appendix).

---- Figure 5 about here ----

But, is the scholarship program important enough to confound our DDD result? It can bias the DDD estimate to zero if it sufficiently drives up years of education for the young cohort. We cannot directly test this because the census data do not provide information on scholarships, but we can get an idea about the maximum possible bias induced by the scholarship program by removing from the young age cohort of each province a share of the best educated children, equal to the province-level share of children that received a scholarship according to the EICV1 data. For instance, since 14% of students are reported to receive a scholarship in Gitarama province, compared to 5% in Gisenyi province, we drop from our sample 14% of the best educated children in the young cohort in Gitarama province and 5% in Gisenyi province. Doing so, we find that the DDD coefficient declines to -0.378, but remains statistically insignificant (Panel D of Table 9). We can therefore conclude that the potential bias induced by the program cannot account for the statistical mismatch between genocide intensity and drop in schooling.[35]

8. The puzzle remains: what accounts for the large drop in schooling?

When we account for potentially confounding factors, the DDD estimate changes, but remains statistically insignificant. Moreover, the DD estimate remains remarkably stable across all the tests we performed. The puzzle thus remains: we cannot attribute the large drop in schooling to the localized effect of the genocide.

[35] Results are robust to dropping the share of the most educated individuals in the young cohort by commune rather than by province as well as to restricting the focus on paternal orphans.

We start this section by documenting this puzzle more carefully, by a spatial and a subsample analysis; then we investigate whether the drop in schooling may be unrelated to armed conflict and instead reflect a regional or national trend; or whether it is driven by other forms of violence that affected Rwanda in the nineties. After excluding these possibilities, we provide arguments in support of the conjecture that the impact of armed conflict in Rwanda was nationwide and thus not confined to the localized effects of the genocide.

(a) The puzzle remains

We visualize the subnational mismatch between the drop in schooling and genocide intensity by comparing the spatial pattern of the estimated drop in schooling with the spatial pattern of the pre-genocide share of Tutsi in the population. Figure 6a plots the quintiles of the commune-level DD estimates obtained by estimating Eq.(1) for each commune *j* separately, using the population census data. The spatial pattern shows clusters of large drops in schooling in the Northwest, scattered throughout the Centre, the South and East. The comparison of Figure 6a with Figure 6b, which plots the share of Tutsi living in each commune in 1991, reveals a lack of correspondence between the distribution of the two variables. For instance, since the share of Tutsi in the northwestern provinces was as low as 1.5% (compared to over 10% in the South), it is unlikely that the large estimated drop in schooling in the Northwest is due to the genocide.

---- Figure 6 about here ----

This is more rigorously shown by Figure 7, which plots the absolute value of the commune-level DD estimates against the commune-level share of Tutsi in 1991. The relationship is

negative, indicating that the drop in schooling is on average smaller in communes with a high share of Tutsi.[36]

---- Figure 7 about here ----

A further confirmation of the mismatch comes from the comparison of the estimated DD coefficient across two subsamples, one for provinces with over 5% Tutsi in their population prior to the genocide and one for the other provinces. The estimates are remarkably similar, at -0.641*** and -0.689***, respectively (not reported).

If not the localized effects of genocide, what is driving the drop in schooling?

(b) A more general regional or national time trend

Could it be that the drop in schooling picked up by the DD model is unrelated to genocide and is instead driven by a time trend? In order to test for this possibility AdW compare the trends in schooling in Rwanda in the nineties with those in neighboring countries (Kenya, Tanzania and Uganda). They demonstrate that years of schooling consistently increased for the Rwandan neighbors, indicating that the observed drop in Rwanda cannot be accounted for by regional events or regional time trends.

Here, we perform another falsification test to verify whether a longstanding national time trend is driving our results. We estimate the DD model for two older cohorts of individuals, which were not exposed to armed conflict at schooling age, i.e. 31-50 and 51-70.[37] The DD

[36] Because Tutsi were underreported in the population census, this pattern may be distorted. We therefore repeat the Figure for Gikongoro province only, using more reliable data on ethnicity coming from the local administration of the Province (see footnote 3). The pattern we find is again negative (not reported).

[37] According to the 1991 population census, less than 1% of the individuals above 23 years were at school in 1991. We assume a similar distribution for 1994, and consider that individuals aged 23 in 1994 were 31 in 2002. The result is in any case very robust to changes in the age categories.

coefficient, is estimated at 0.148***, significant at 1% (not reported), indicating that, if anything, prior the genocide there was an upward national trend in schooling.

(c) Other forms of violence

The 1994 genocide was extremely violent (around 800.000 victims), but short-lived (around 100 days). Other less intense, but longer-lasting forms of violence took place in Rwanda during the nineties, including civil war, revenge killings, insurgency and counter-insurgency operations. Could it be that the drop in schooling picked up by the DD model is driven by these other events? To tentatively test this hypothesis, we rely on the scarcely available data that document other forms of violence in Rwanda.

The first dataset is taken from Serneels and Verpoorten (2013) and contains an index that records excess mortality caused by episodes of violence different from the genocide. The index was generated by relying on the two population census rounds and on eleven excess mortality proxies.[38] The second dataset contains the number of extrajudicial killings of civilians during the (counter)insurgency operations that took place till the late nineties in the Northwest of Rwanda. Data are collected from four Amnesty International Reports (Amnesty International, 1996, 1997a, 1997b, 1998).[39] While the first dataset has national coverage, the second dataset only contains information for the two provinces in the Northwest.

Table 11 reports the results using these two alternative conflict proxies. The DDD estimates are negative, and in general statistically insignificant (for some age groups they turn

[38] See Serneels and Verpoorten (2013) for further details on the construction of this variable.
[39] Verpoorten (2012) provides details on the compilation of this dataset.

significant).[40] These results suggest that other forms of violence may partly account for the drop in schooling, but the fact that the DD coefficient remains negative, large and significant at 1%, indicates that the large drop in schooling remains unexplained, also by other forms of violence.

(d) The cycle of violent events in the 1990s had above all a nationwide impact

So far we have shown that Rwanda experienced a large drop in schooling during the violent decade of the nineties. We have excluded the possibility that this drop is part of a more general time trend. We have also excluded our first best guess, i.e. that the drop relates to the localized effects of the genocide. Finally, we have shown that the large drop can neither solely be attributed to the localized effects of other specific forms of violence that took place in the nineties.

Putting these pieces of evidence together leads us to conjecture that the impact of the genocide on schooling was nationwide, because its disruptive effect was nationwide, or because it operated through national channels, or because genocide coincided with other forms of violence leaving no single area in Rwanda unaffected. Admittedly, this "residual" explanation is somewhat *ad hoc*. We do however believe there is some basis for this conjecture.

First, a 1994 report from the Ministry of Education (MINEPRISEC/MINESUPRES, 1994) indicates that the destruction of schools was widespread; 1186 out of 1836, or 65%, of the schools were reported to be damaged, needing urgent repair, while only 648 schools were fully operational. Moreover, most of the schools were reported to be looted and pillaged and 25% of them were still occupied by military forces or internally displaced people. Unfortunately, we do

[40] The coefficient of the Excess Mortality Index turns significant whenever considering the alternative age cohorts 8-17 or 6-22. However, the DD coefficient remains negative, large and significant at 1%, thus confirming our conclusion.

not have detailed information on the actual location of the damaged schools. We can however derive information from a nationally representative community survey collected in 1999/2000 (EICV1). The information on schools and schools built since 1994, reveals that communities more affected by the genocide were not less likely to be in the proximity of an operating primary school, and were not more likely to be the location of reconstruction or renovation works of a school building since 1994.[41] This finding suggests that school buildings across the genocide-affected and other communities were equally likely to have incurred damage.

Second, besides school infrastructure, teachers had become a very scarce resource: many were killed, because they were Tutsi or part of the moderate Hutu elite; several others who were part of the Hutu elite had actively participated in the killings turning against fellow teachers, neighbors and pupils; and still others had moved abroad or to urban centers (Obura, 2003). The result was "*the total erosion of faith in the education system*", with less than half of qualified teachers remaining in the primary system after the conclusion of the conflict (*ibidem*, p.48).[42]

Third, the conflicts of the nineties heavily impacted the government budget, drying up resources for education.

Fourth, many people, from all areas in Rwanda, fled the advancing RPF army fearing revenge actions against Hutu. They ended up in refugee camps abroad, or in camps within Rwanda located in a "safe zone" set up by the French intervention *Opération Turquoise* in the

[41] Results are based on regressions in which the dependent variable is the answer to one of the following questions: "is there a primary school in the village?", "What is the distance to the closest primary school?" and "Is there a school built after the 4th of July 1994 in the cell?". The regressors include one measure of genocide intensity (results are robust to using any measure) and province FE. Results are not reported, but available on request.

[42] When looking at the presence of teachers, relying on the sector of employment derived from the census data, we do find that communes more affected by the genocide had a significantly lower number of teachers (as a share of the active population) than less affected communes, after controlling for the share of teachers in 1991 levels. Although this may negatively affect the supply of schooling in the genocide-affected communes, the results of our paper show that it does not result in a larger drop in schooling in genocide-affected areas (section 7) and that it cannot account for the large drop in schooling in areas that were not affected by the genocide (section 8).

Southwestern part of Rwanda. The externally displaced, who were estimated at 2 million, or about 25% of the Rwandan population, only returned to Rwanda at a slow pace from 1996 onwards (UNHCR, 2000). When Opération Turquoise ended on 22 August, some 390,000 out of the initial 1.5 million internally displaced remained in 33 camps. The forced closures of these remaining camps took place between August 1994 and April 1995 (Prunier, 1998). Unfortunately, we do not have data on refugee status to test for this channel, but given the large numbers of people staying in camps for several months or even years, the refugee crisis in itself is likely to account for part of the drop in schooling.

9. Conclusion

By now, a large number of studies have documented the impact of armed conflict on schooling. Most studies rely on DHS data or other survey data that are representative at the national level. Our paper relied on two waves of population census data, which - compared to the DHS - have the advantages of being representative at the levels of small administrative units as well as subgroups of the population.

Relying on these richer data, we replicated the main result reported in the widely cited study by Akresh and de Walque (2011). Our replication confirmed a drop in schooling of approximately 18% for children exposed to armed conflict in Rwanda. The estimated drop reached up to 25%, when older cut-off ages are used for the young age cohort, partly because there also was a sizable drop in secondary schooling.

In order to shed more light on the causes of the observed drop, we provided novel evidence on the relative importance of school initiation, slow grade progression and dropouts, showing that the latter two account for the drop in primary schooling, while school initiation is

the main factor explaining the drop in secondary schooling. The finding that a sizeable share of the observed decline in primary schooling is driven by delays is hopeful, as it leaves room for rapid recovery. In terms of implications for future research, these findings suggest that empirical studies should give due attention to the time elapsed since the end of the conflict. It would for instance be possible to find a large negative impact of conflict on schooling in the early postwar years but no impact or even a positive impact further down the road.

In their paper, AdW reported an additional negative effect of armed conflict on children from non-poor households (versus those from poor households) and on exposed boys (versus girls). When correcting their empirical specification by including all constitutive components of the interaction terms, our narrow replication resulted in a much smaller non-poor effect and did not yield any gender effect. Using a much larger sample, the broad replication yielded no conclusive results regarding the heterogeneous impact across poor and non-poor households and an additional – albeit small – negative effect for exposed girls.

Also in contrast to AdW, we could not attribute the drop in schooling to the localized effects of the genocide, even when using data with complete geographic coverage and finer genocide intensity measures. It may seem counterintuitive that one of the largest mass killing of the 21st century does not stand out in its effect on schooling. We ruled out that this counterintuitive result stems from pre-war regional trends, nonrandom displacement, selective killings, or targeted assistance to genocide survivors: correcting for these factors did not explain away the large drop in schooling, which is observed also in areas where no or very few Tutsi lived prior to the genocide.

These results suggest that the impact of the genocide on the drop in schooling was nationwide; because it largely operated through national channels, or because the genocide

triggered other disruptive events which affected every corner of the country. By the time massive violence had stopped, the majority of school buildings were destroyed; the majority of teachers were displaced, killed or imprisoned; the government budget was virtually zero; and 30% of the population was internally or externally displaced, living in camps without proper access to schooling. Furthermore, other forms of violence, including civil war, revenge killings, insurgency and counter-insurgency, took place in the nineties, leaving no area of Rwanda violence-free. Combined, these events affected the entire country, and - although very different in nature compared to the genocide - these events may have been equally disruptive to schooling.

This conclusion has relevant policy implications. Scholarships have been directed to genocide-affected areas and survivors of the genocide, being channeled through the various associations for survivors. Given the history of ethnic discrimination in Rwanda and its role in triggering and intensifying violence, such bias is not to be taken lightly. There is a danger that post-war reconstruction that is exclusively directed to genocide survivors and overlooks the needs of other victims might not only slow down the recovery of educational attainment, but may also reinforce group identities, potentially feeding ethnic grievances.

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Acknowledgements

We are grateful to Nathan Fiallia, Giulia La Mattina, Andrea Ruggeri, Olga Shemyakina, Prakarsh Singh and participants at the Jan Tinbergen European Peace Science Conference in Milan for very helpful comments. We owe thanks to Nik Stoop for excellent research assistance and to the Rwandan National Census Service and Minnesota Population Center for making available the data used in this study. All errors and opinions expressed remain our own.

Figure 1. Years of schooling, across all ages of the age group 6-35

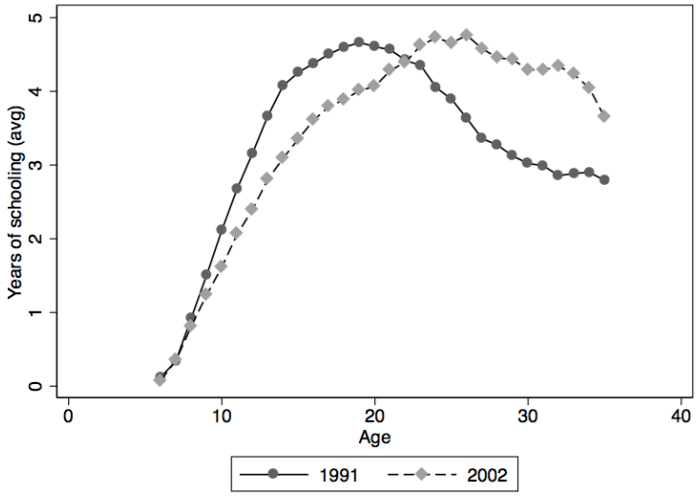


Figure 2. Years of schooling, across age and rural-urban

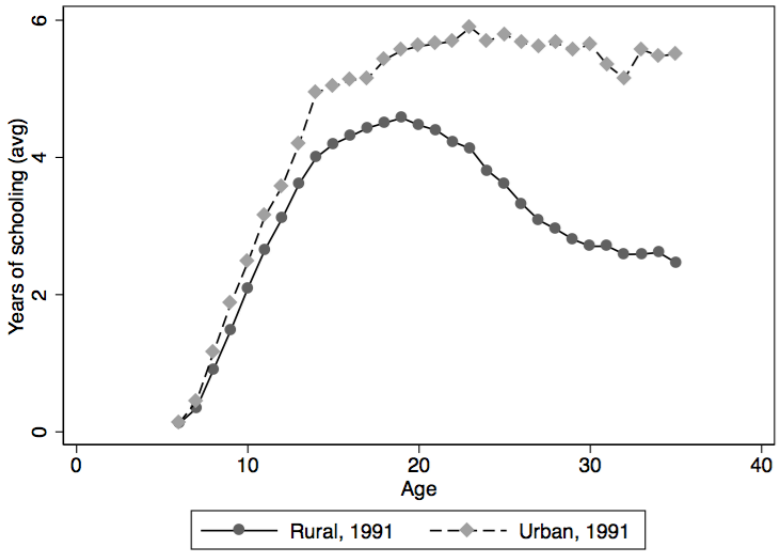
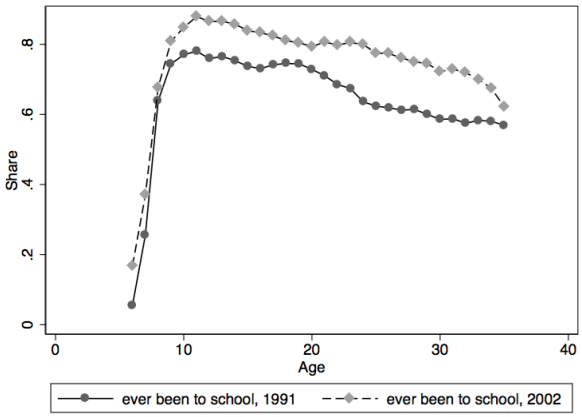
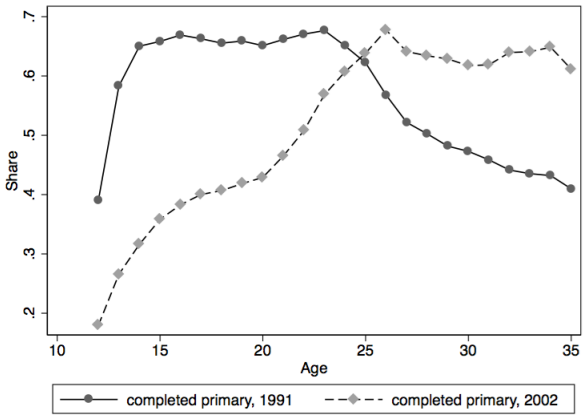


Figure 3. School initiation, drop-out or slow grade progression?

(a) Share of individuals that have ever been to school, by age



(b) Share of individuals that have completed primary school, among those that have ever attended it, but that are no longer enrolled in primary school, by age



(c) Average age by grade, for student population only

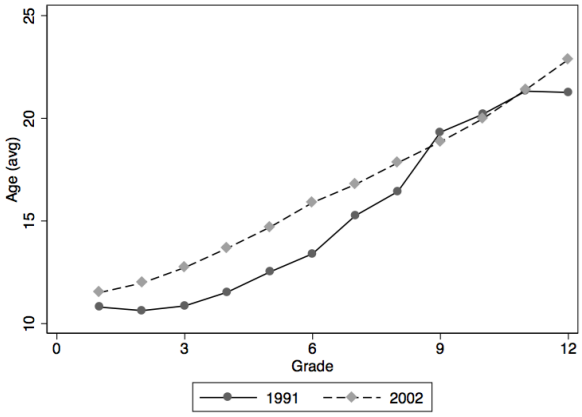


Figure 4. Schooling across ethnicity in 1991

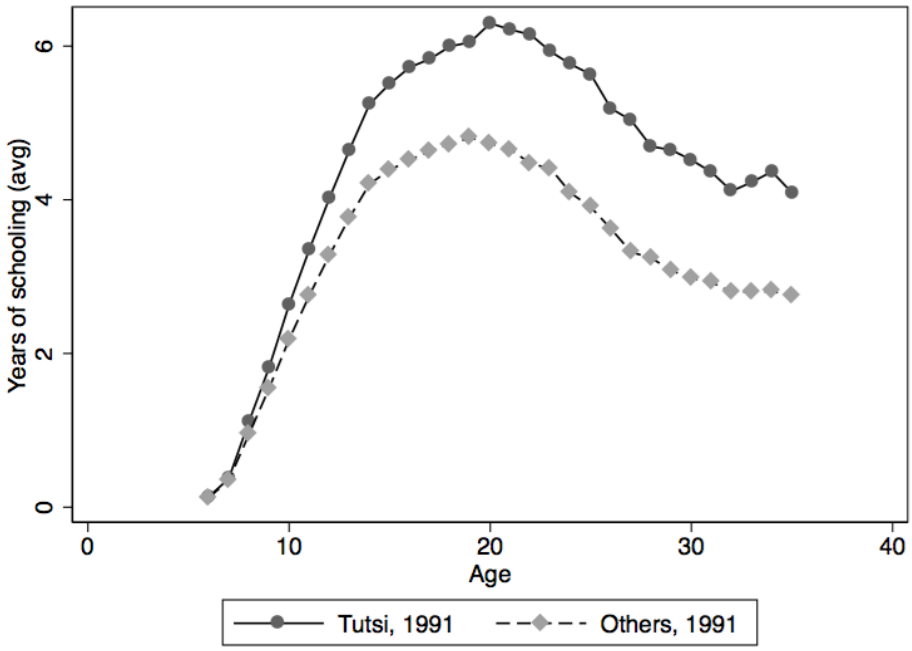


Figure 5. The relationship between the pre-war commune-level share of Tutsi and the share of students 6-35 enjoying a scholarship, as reported in a 1999/2000 nationwide survey

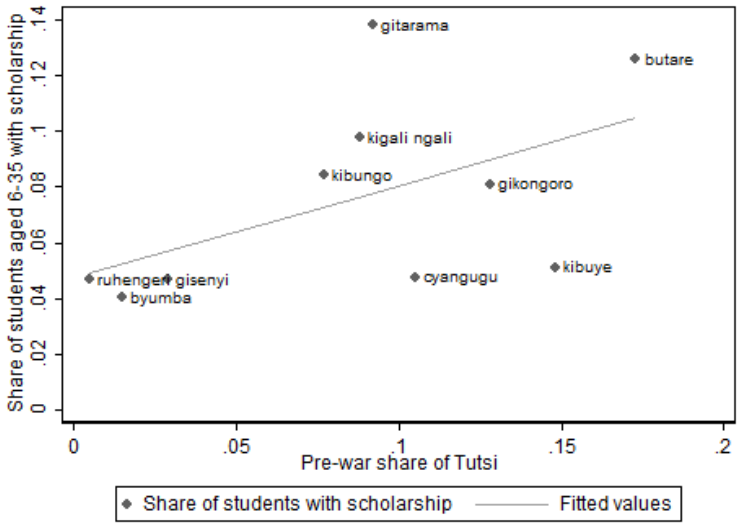
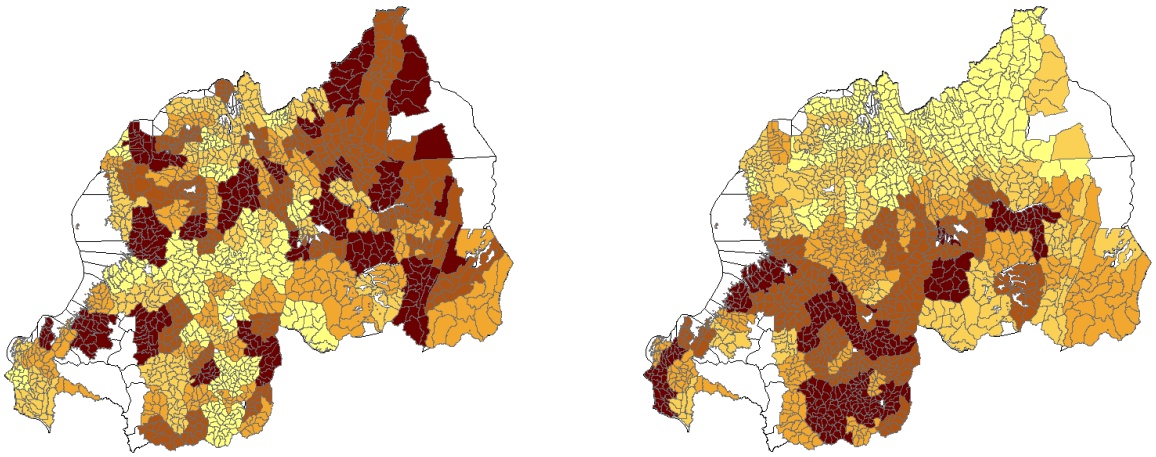


Figure 6. Comparison of the spatial pattern of DD estimates of the schooling deficit with the pre-genocide share of Tutsi in a commune

(a) DD estimates, young age cohort: 6-15 (b) Pre-war commune-level share of Tutsi



Top quintile (= largest schooling deficit or highest share of Tutsi) in darkest shade, calculated for each commune separately, using log(schooling) as a dependent variable to account for the different levels of schoolings between communes. The map is taken from a shape file of the Rwandan administrative sectors (which is one level below the communes, but for which no shape file exists). The areas in white are left out of the analysis. They include the national park, forest areas and lakes.

Figure 7. The relationship between the pre-war commune-level share of Tutsi and the absolute values of the commune level difference-in-difference estimates of the drop in schooling

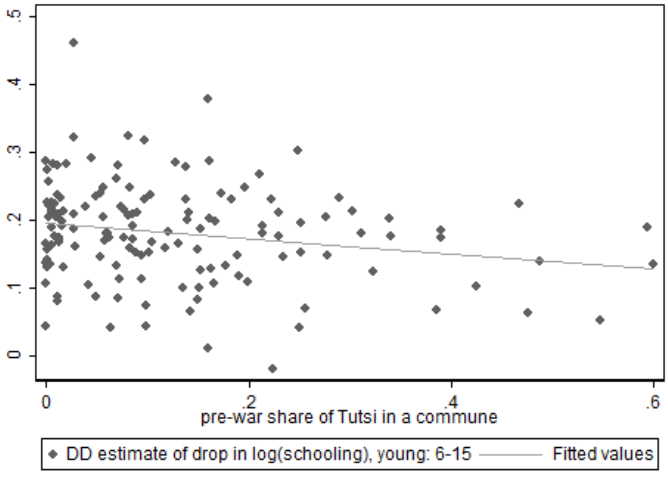


Table 1. Summary Statistics

Variable	Full Sample					Pre-conflict	Post-conflict
	Observations	Mean	Std Deviation	Min	Max	mean	mean
PANEL A: DHS							
Years of schooling	45400	3,296	3,293	0	21	3,420	3,212
Post-War Round	45400	0,597	0,491	0	1	0	1
Young Cohort	45400	0,489	0,500	0	1	0,477	0,497
Female	45400	0,523	0,499	0	1	0,514	0,529
Non-poor	45400	0,432	0,495	0	1	0,571	0,337
Age of HH Head	45400	42,975	13,938	7	97	44,049	42,249
Highest Education - Any HH Member	45400	5,121	4,006	0	21	5,369	4,954
Number of Children Under 5	45400	0,818	0,837	0	5	0,855	0,793
Rural	45400	0,781	0,414	0	1	0,830	0,747
Conflict Intensity (=Measure A)	45400	0,262	0,211	0,125	1	-	-
Conflict Intensity (=Measure B)	45400	0,305	0,460	0	1	-	-
Conflict Intensity (=Measure C)	45400	8,992	5,766	3	20	-	-
PANEL B: Census							
Years of schooling	907026	3,050	2,992	0	20	3,008	3,089
Post-War Round	907026	0,513	0,500	0	1	0	1
Young Cohort	907026	0,460	0,498	0	1	0,474	0,448
Female	907026	0,520	0,500	0	1	0,509	0,531
Non-poor	907026	0,554	0,497	0	1	0,482	0,621
Age of HH Head	907026	42,556	13,966	6	108	42,783	42,341
Highest Education - Any HH Member	907026	4,378	3,674	0	20	4,337	4,417
Number of Children Under 5	907026	0,910	0,894	0	9	1,008	0,816
Rural	907026	0,854	0,331	0	1	0,864	0,845
Been to School	885283	0,697	0,459	0	1	0,631	0,763
Been to Secondary	907026	0,127	0,333	0	1	0,157	0,098
Student	907026	0,241	0,428	0	1	0,168	0,311
Completed primary school	907026	0,240	0,427	0	1	0,254	0,226
Completed secondary school	907026	0,012	0,109	0	1	0,008	0,016
Conflict Intensity (=Measure A)	907026	23,947	19,598	12	96	-	-
Conflict Intensity (=Measure B)	907026	0,275	0,447	0	1	-	-
Conflict Intensity (=Measure C)	907026	8,789	6,005	3	20	-	-
Conflict Intensity (= Share Tutsi)	907026	0,114	0,119	0	0,6	-	-
Conflict Intensity (= Perpetrators)	907025	0,069	0,045	0,002	0,197	-	-
Conflict Intensity (= Mass Graves)	907026	0,491	0,803	0	4	-	-
Conflict Intensity (= (log) dist to Mass Grave)	907026	2,260	0,658	0,677	4,034	-	-
Conflict Intensity (=Excess Mortality Index)	907026	0,389	0,175	0	1	-	-
Conflict Intensity (=Extrajudicial Killings)	190694	0,409	0,643	0	2,287	-	-
Migrant (8 year)	907026	0,165	0,371	0	1	0,118	0,210
Orphan	907026	0,230	0,421	0	1	0,137	0,318

Notes: For DHS data, pre-conflict refers to 1992 and post-conflict to 2000. For Census data, pre-conflict refers to 1991 and post-conflict to 2002. In both cases the sample is limited to idnividuals aged 6 to 35. See the paper for details concerning the specific variables.

Table 2. Narrow and Broad replication of basic result

Dependent Variable: Years of schooling		Replication			Replication	
	AdW	DHS data	Census data	AdW	DHS data	Census data
	(1)	(2)	(3)	(4)	(5)	(6)
Young Cohort * Post-war round	-0.555***	-0.504***	-0.584***	-0.421***	-0.494***	-0.660***
	(0.116)	(0.131)	(0.033)	(0.097)	(0.102)	(0.029)
Post-War Round	-0,123	0.092	0.296***	-0.232***	0.231***	0.153***
	(0.140)	(0.157)	(0.042)	(0.064)	(0.065)	(0.020)
Young Cohort	-2.249***	-2.541***	-1.776***			
	(0.085)	(0.104)	(0.069)			
Controls X						
Female				-0.113***	-0.074***	-0.132***
				(0.026)	(0.026)	(0.023)
Non-poor				0.165***	0.294***	0.275***
				(0.024)	(0.031)	(0.012)
Age of HH Head				0.005***	0.009***	0.008***
				(0.001)	(0.001)	(0.000)
Highest Education - Any HH Member				0.446***	0.386***	0.362***
				(0.006)	(0.004)	(0.002)
Number of Children Under 5				-0.113***	-0.116***	-0.053***
				(0.013)	(0.016)	(0.011)
Rural				-0.270***	-0.408***	-0.132***
				(0.103)	(0.047)	(0.031)
Child Age FE	No	No	No	Yes	Yes	Yes
Administrative Unit FE	No	No	No	Province	Province	Province
Observations	45,642	45,400	907,026	45,642	45,400	907,026
R-squared	n.a.	0.188	0.122	n.a.	0.521	0.425

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3. Alternative age cohorts and the impact on secondary schooling

Dependent Variable: Years of schooling			
Young Cohort:	8-17 years	6-22 years	14-22 years
Old Cohort:	18-37 years	23-35 years	23-35 years
	(1)	(2)	(3)
Young Cohort * Post-war round	-0.931***	-1.189***	-1.083***
	(0.032)	(0.033)	(0.034)
Post-War Round	0.306***	0.693***	0.581***
	(0.018)	(0.024)	(0.018)
Young Cohort	-1.525***	-2.328***	1.078***
	(0.088)	(0.097)	(0.073)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province
Observations	839,691	907,026	566,883
R-squared	0.391	0.429	0.419

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4. Poor versus non-poor

Dependent Variable: Years of schooling	AdW	DHS data	Census data	
	(1)	(2)	(3)	(4)
Young Cohort * Post-war round	0.160*	-0.729***	-0.526***	-0.478***
	(0.083)	(0.073)	(0.026)	(0.028)
Post-War Round	0.203***	0.338***	0.110***	0.086***
	(0.059)	(0.054)	(0.016)	(0.018)
Non-Poor * (Young Cohort * Post-war round)	-1.223***	-0.399***	-0.013	0.062*
	(0.064)	(0.145)	(0.033)	(0.034)
Non-Poor * Young Cohort		-1.526***	-0.943***	-0.895***
		(0.110)	(0.069)	(0.046)
Non-Poor * Post-war round		0.149	-0.036	-0.095***
		(0.094)	(0.024)	(0.024)
Urban * (Young Cohort * Post-war round)				-0.184
				(0.138)
Urban * Young Cohort				-0.733***
				(0.217)
Urban * Post-war round				0.352***
				(0.056)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province
Observations	45,642	45,400	907,026	907,026
R-squared	n.a.	0.537	0.431	0.433

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5. Girls versus boys

Dependent Variable: Years of schooling	AdW	DHS data	Census data	
	(1)	(2)	(3)	(4)
Young Cohort * Post-war round	- 0.535***	-0.478***	-0.578***	-0.455***
	(0.101)	(0.115)	(0.036)	(0.026)
Post-War Round	0.235***	0.230***	0.043	-0.028
	(0.064)	(0.079)	(0.028)	(0.021)
Female * (Young Cohort * Post-war round)	0.219***	-0.039	-0.162***	-0.172***
	(0.044)	(0.098)	(0.026)	(0.025)
Female * Young Cohort		0.252***	0.331***	0.329***
		(0.081)	(0.033)	(0.033)
Female * Post-war round		0.009	0.219***	0.230***
		(0.086)	(0.025)	(0.023)
Urban * (Young Cohort * Post-war round)				-0.146
				(0.169)
Urban * Young Cohort				-0.971***
				(0.270)
Urban * Post-war round				0.354***
				(0.069)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province
Observations	45,642	45,400	907,026	907,026
R-squared	n.a.	0.537	0.425	0.429

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6. School initiation, slow grade progression and drop-outs

PRIMARY SCHOOLING, Dependent Variable:	(1) Been to school	(2) Completed primary	(3) Age Students
Young Cohort:	6-15	14-22	10-18
Old Cohort:	16-35	23-35	
Young Cohort * Post-war round	-0.007	-0.285***	
	(0.009)	(0.012)	
Post-War Round	0.098***	0.065***	1.708***
	(0.005)	(0.008)	(0.063)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Administrative Unit FE	Province	Province	Province
Observations	894,602	387,833	131,890
R-squared	0.256	0.249	0.314
SECONDARY SCHOOLING, Dependent Variable:	Been to secondary	Completed secondary	Age Students
Young Cohort:	14-22	18-26	18-26
Old Cohort:	23-35	27-35	
Young Cohort * Post-war round	-0.400***	0.045***	
	(0.013)	(0.009)	
Post-War Round	-0.014	0.066***	0.209***
	(0.008)	(0.004)	(0.065)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Administrative Unit FE	Province	Province	Province
Observations	210,842	72,179	16,252
R-squared	0.288	0.555	0.173

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. PANEL A: the sample considered in the second column is restricted to individuals that have completed at least one year of primary schooling, but that are not enrolled in primary school any more; the sample considered in the third column for primary school is restricted to students attending class 1 to 6. PANEL B: the sample considered in the first column is restricted to individuals that have completed primary school; the sample considered in the second column is restricted to individuals that have completed at least one year of secondary schooling, but that are not enrolled in secondary school any more; the sample considered in the third column is restricted to students attending class 7 to 12.

Table 7. Triple differenced regression with province-level conflict intensity measures

Dependent Variable: Years of schooling	Panel A: AdW			Panel B: DHS data			Panel C: Census data		
Conflict Intensity Measure:	A	B	C	A	B	C	A	B	C
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Conflict Intensity * (Young * Post-war)	-0.024*	-0.329*	-0.023*	0.744**	-0.892***	0.005	0.003**	0.094	0.001
	(0.014)	(0.196)	(0.013)	(0.296)	(0.220)	(0.016)	(0.001)	(0.060)	(0.003)
Conflict Intensity * Young Cohort	n.a.	n.a.	n.a.	-0.062	0.176	0.022*	-0.002	0.011	-0.003
				(0.238)	(0.164)	(0.012)	(0.002)	(0.083)	(0.005)
Conflict Intensity * Post-war round	n.a.	n.a.	n.a.	-0.611***	0.317**	-0.020*	-0.002*	-0.024	-0.005**
				(0.227)	(0.142)	(0.011)	(0.001)	(0.037)	(0.002)
Young Cohort * Post-war round	n.a.	n.a.	n.a.	-0.688***	-0.230**	-0.529***	-0.731***	-0.683***	-0.672***
				(0.140)	(0.117)	(0.187)	(0.038)	(0.032)	(0.034)
Post-War Round	n.a.	n.a.	n.a.	0.391***	0.123*	0.412***	0.191***	0.159***	0.198***
				(0.094)	(0.074)	(0.124)	(0.027)	(0.023)	(0.025)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province	Province	Province	Province	Province	Province
Observations	45,642	45,642	45,642	45,400	45,400	45,400	907,026	907,026	907,026
R-squared	n.a.	n.a.	n.a.	0.521	0.524	0.521	0.425	0.425	0.425

Notes: Robust standard errors in parentheses, clustered at the enumeration level for the DHS data and at the commune level for the census data.All conflict Intensity Measures are defined at the Province level. *Measure A* is represented by the proportion of days during which killings occurred in a province in the months April-June 1994; *Measure B* is instead an indicator variable taking one for the three provinces with the highest number of killings in 1994; finally *Measure C* records the number of mass graves and memorials per province. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8. Triple differenced regression with commune-level conflict intensity measures

Dependent Variable: Years of schooling				
Conflict Intensity Measure:	Share Tutsi	Share Perpetrators	Mass Graves	Dist to Mass Grave
	(1)	(2)	(3)	(4)
Conflict Intensity * (Young * Post-war)	-0.089	0.852*	-0.044	0.008
	(0.219)	(0.480)	(0.035)	(0.039)
Conflict Intensity * Young Cohort	-1.268***	-1.039	-0.160***	0.375***
	(0.362)	(1.059)	(0.059)	(0.085)
Conflict Intensity * Post-war round	0.102	-0.830***	0.011	0.009
	(0.160)	(0.308)	(0.024)	(0.032)
Young Cohort * Post-war round	-0.666***	-0.723***	-0.643***	-0.695***
	(0.025)	(0.040)	(0.026)	(0.099)
Post-War Round	0.151***	0.212***	0.149***	0.142*
	(0.020)	(0.030)	(0.018)	(0.079)
Conflict Intensity (= Share Tutsi)	0.885***			
	(0.226)			
Conflict Intensity (= Perpetrators)		1.367**		
		(0.671)		
Conflict Intensity (= Mass Graves)			0.083**	
			(0.033)	
Conflict Intensity				-0.257***
(= Distance to Mass Grave - log)				(0.049)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province
Observations	907,026	907,024	907,026	907,026
R-squared	0.426	0.425	0.425	0.427

Notes: Robust standard errors in parentheses, clustered at the commune level. All conflict Intensity Measures are defined at the Commune level. *Share Tutsi* is the proportion of Tutsi living in the commune in the pre-war population; *Share Perpetrator*s is the share of genocide suspects identified in the Commune; *Mass Graves* is the number of mass graves in the Commune; *Dist to Mass Grave* is and the distance to the nearest mass grave calculated from the commune centroid. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9. Confounding factors

Dependent Variable: Years of schooling	Panel A: province factors		Panel B: Assign Migrants to Previous Commune		Panel C: drop 75% best educated Tutsi		Panel D: drop best educated individuals in young cohort proportional to scholarships	
	DD	DDD	DD	DDD	DD	DDD	DD	DDD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Young Cohort * Post-war round	-0.605***	-0.624***	-0.624***	-0.628***	-0.711***	-0.692***	-0.802***	-0.780***
	(0.067)	(0.093)	(0.027)	(0.026)	(0.032)	(0.027)	(0.031)	(0.027)
Post-War Round	0.155***	0.165***	0.120***	0.117***	0.217***	0.174***	0.154***	0.152***
	(0.045)	(0.060)	(0.023)	(0.021)	(0.023)	(0.020)	(0.020)	(0.020)
Conflict Intensity * (Young * Post-war)		0.047		-0.099		-0.213		-0.378
		(0.243)		(0.181)		(0.224)		(0.260)
Conflict Intensity * Young Cohort		-1.065***		-1.267***		-1.149***		-1.259***
		(0.249)		(0.356)		(0.367)		(0.362)
Conflict Intensity * Post-war round		-0.014		0.106		0.408**		0.104
		(0.166)		(0.162)		(0.170)		(0.159)
Conflict Intensity (= Share Tutsi)		0.811***		0.879***		0.587**		0.917***
		(0.208)		(0.212)		(0.227)		(0.225)
Controls X	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province	Province	Province	Province	Province
Complete set of province factors	Yes	Yes	No	No	No	No	No	No
Observations	907,026	907,026	864,129	864,129	880,235	880,235	891,412	891,412
R-squared	0.429	0.429	0.420	0.421	0.422	0.423	0.429	0.430

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. The conflict Intensity variable considered in the regression is the Share of Tutsi living in the Commune in 1991.Regressions in Panel A include a complete set of province trends and province interaction terms (i.province*young*postwar, i.province*young, i.province*postwar). Regressions in Panel B consider individuals that migrated between 1994 and 2002 as belonging to their commune of origin. Regressions in Panel C exclude from the 1991 population 75% of Tutsi, randomly selected. Regressions in Panel D exclude from each province a share of individuals in the young cohort of the 2002 population census equal to the share of students reported to have a scholarship according to the EICV1 dataset.

Table 10. Schooling fees FARG

Year	Number of beneficiaries		Amount
	Secondary	University	spent (RWF)
98-99	24 147	295	2 523 823 954
99-2000	26 474	978	2 082 207 374
2000-01	33 816	732	3 588 222 213
2001-02	33 929	1 275	4 333 510 776
2002-03	38 943	1 190	5 732 070 752
2003-04	43 952	1 081	3 776 603 240
2005	44 680	1 183	5 107 782 847
2006	47 371	1 317	7 090 147 893
2008	50 011	2 137	8 414 730 973
2009	52 737	3 844	9 558 351 570
2010	41 003	5 350	11 598 015 692
TOTAL			63 805 467 284

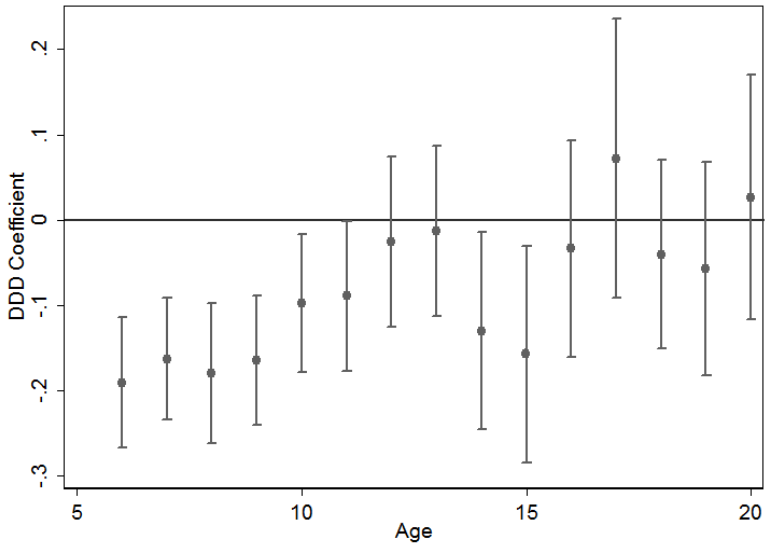
Table 11. Other forms of violence

Dependent Variable: Years of schooling	Panel A: Non-genocide excess mortality index		Panel B: Extrajudicial killings in the NorthWest	
	DD	DDD	DD	DDD
	(1)	(2)	(3)	(4)
Young Cohort * Post-war round	-0.660***	-0.584***	-0.610***	-0.582***
	(0.029)	(0.054)	(0.045)	(0.051)
Post-War Round	0.153***	0.157***	0.100***	0.093**
	(0.020)	(0.034)	(0.033)	(0.038)
Conflict Intensity * (Young * Post-war)		-0.196		-0.067
		(0.137)		(0.043)
Conflict Intensity * Young Cohort		-0.318		0.052
		(0.258)		(0.104)
Conflict Intensity * Post-war round		-0.012		0.015
		(0.091)		(0.025)
Conflict Intensity (=Excess Mortality Index)		0.139		
		(0.170)		
Conflict Intensity (=Extrajudicial Killings)				-0.090*
				(0.052)
Controls X	Yes	Yes	Yes	Yes
Child Age FE	Yes	Yes	Yes	Yes
Administrative Unit FE	Province	Province	Province	Province
Observations	907,026	907,026	190,694	190,694
R-squared	0.425	0.425	0.416	0.416

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. The conflict Intensity variable considered in Panel A is the excess mortality caused by episodes of violence different from the genocide, as constructed by Verpoorten (2012). The conflict Intensity variable considered in Panel B is the number of extrajudicial killings of civilians during the (counter)insurgency, collected from four Amnesty International Reports. In Panel B the sample is restricted to the provinces of Gisenyi and Ruhengeri, where (counter)insurgency took place.

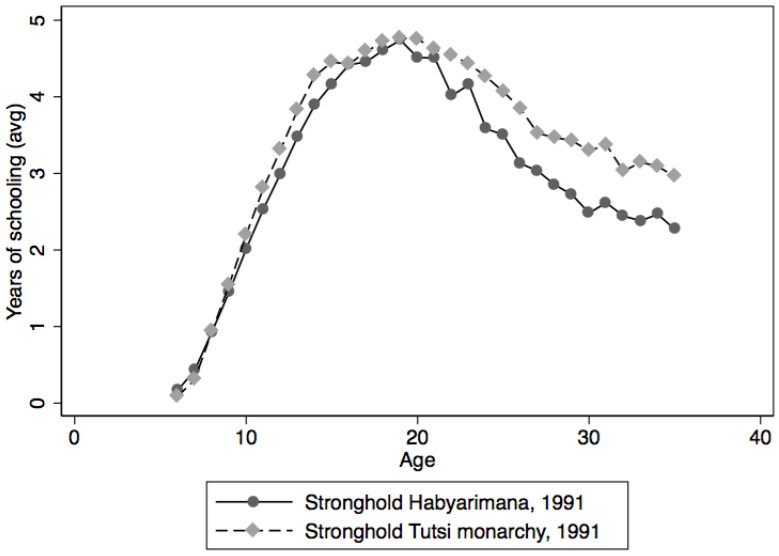
APPENDIX

Figure A1. DDD coefficient by age



Notes: The figure shows the point estimates and the 95% CI for the DDD coefficients of the interaction terms obtained from a regression similar to the one reported in column (3) of Table 4, in which the *Young* cohort has been replaced by the individual years, from 6 to 20 i.e. all the triple interactions *female*postwarround*age6* etc are included, as well as all partial interactions.

Figure A2. Regional trends in schooling in 1991



Notes: The stronghold of Habyarimana includes the northwestern provinces Gisenyi and Gitarama. The stronghold of the Tutsi monarchy covers Butare and Gitarama.

Figure A3. Years of schooling, across age and migrants vs. non-migrants

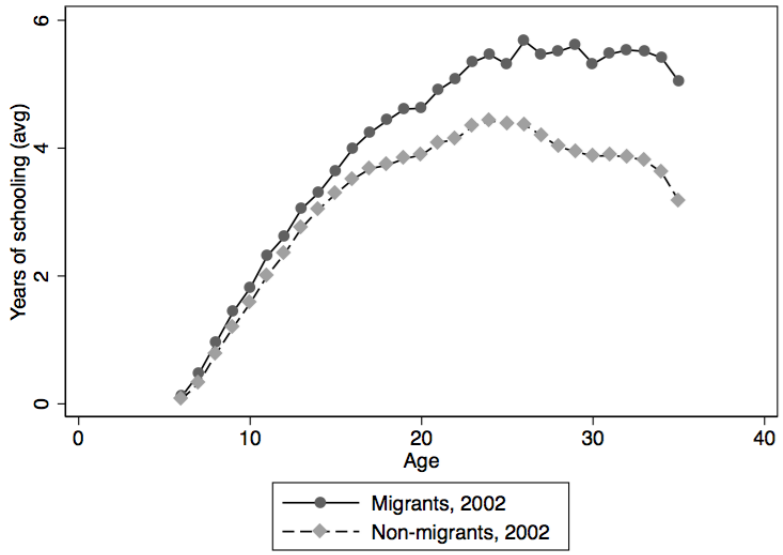


Figure A4. Years of schooling, across age, high or low share of Tutsi in 1991 and paternal orphans vs. others

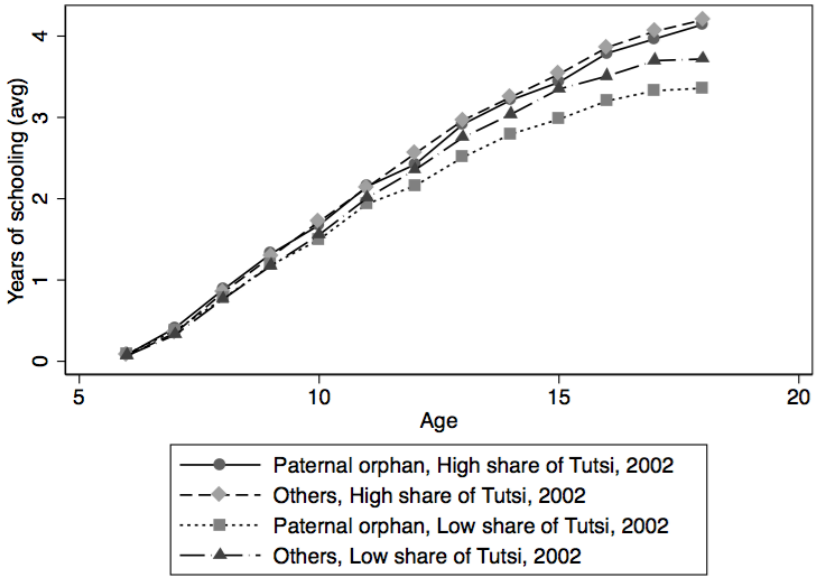


Table A1. School initiation, slow grade progression and drop-outs – girls versus boys

PRIMARY SCHOOLING, Dependent Variable:	(1) Been to school	(2) Completed primary	(3) Age Students
Young Cohort:	6-15	14-22	10-18
Old Cohort:	16-35	23-35	
Young Cohort * Post-war round	0.013*	-0.272***	
	(0.007)	(0.012)	
Post-War Round	0.070***	0.063***	1.747***
	(0.004)	(0.008)	(0.069)
Female * (Young Cohort * Post-war round)	-0.039***	-0.128***	
	(0.007)	(0.011)	
Female * Young Cohort	0.060***	0.053***	
	(0.010)	(0.005)	
Female * Post-war round	0.055***	0.003	-0.078***
	(0.005)	(0.006)	(0.023)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Administrative Unit FE	Province	Province	Province
Observations	894,602	387,833	131,890
R-squared	0.256	0.250	0.314
SECONDARY SCHOOLING, Dependent Variable:	Been to secondary	Completed secondary	Age Students
Young Cohort:	14-22	18-26	18-26
Old Cohort:	23-35	27-35	
Young Cohort * Post-war round	-0.435***	0.060***	
	(0.009)	(0.011)	
Post-War Round	0.021**	0.051***	0.220**
	(0.008)	(0.010)	(0.072)
Female * (Young Cohort * Post-war round)	0.078***	-0.034***	
	(0.015)	(0.010)	
Female * Young Cohort	-0.083***	0.044***	
	(0.012)	(0.008)	
Female * Post-war round	-0.076***	0.034*	-0.027
	(0.008)	(0.019)	(0.038)
Controls X	Yes	Yes	Yes
Child Age FE	Yes	Yes	No
Grade FE	No	No	Yes
Administrative Unit FE	Province	Province	Province
Observations	210,842	72,179	16,252
R-squared	0.289	0.555	0.173

Notes: Robust standard errors in parentheses, clustered at the commune level. * significant at 10%, ** significant at 5%, *** significant at 1%. PANEL A: the sample considered in the second column is restricted to individuals that have completed at least one year of primary schooling, but that are not enrolled in primary school any more; the sample considered in the last column for primary school is restricted to students attending class 1 to 6. PANEL B: the sample considered in the first column is restricted to individuals that have completed primary school; the sample considered in the second column is restricted to individuals that have completed at least one year of secondary schooling, but that are not enrolled in secondary school any more; the sample considered in the last column is restricted to students attending class 7 to 12.